PLATFORM EMERGENCE IN DOUBLE UNKNOWN: COMMON CHALLENGE STRATEGY

Olga KOKSHAGINA[1,2], Pascal LE MASSON[1], Benoit WEIL[1], Patrick COGEZ[2]
[(1)]MINES PARISTECH, CGS - Center for scientific management
60 Boulevard Saint-Michel, 75272 Paris Cedex 06, France
olga.kokshagina@mines-paristech.fr
[(2)]Technology R&D, STMicroelectronics, Crolles, France
850, rue Jean Monnet
F-38926 Crolles Cedex

Submission number 141
Keywords Risk management, uncertainty, platform emergence

Abstract

Context and Research questions

The proposed paper deals with platform emergence in double unknown situations when technology and markets are highly uncertain. The interest in technological platform development to enable creation of products and processes that support present and future development of multiple options is widely recognised by practitioners and academics (Gawer 2009, Baldwin 2008, Baldwin and Clark 1997, Meyer and Utterback 1993, etc). The existing literature considers already existing platforms and the development is based on exploiting this common platform core to build future markets and technological derivatives. However, when we are in double unknown situations, markets and technologies are highly uncertain and neither options, nor platform core are known. Thus, how can one ensure platform emergence in double unknown?

The first identified in the literature strategy consists of progressive emergence of common core. Generally this leads first to conduct exploration projects to create new knowledge and formulate platform and second, to reuse identified platform to construct platform derivatives (series of projects generate the technology platform with associated derivates) (Sanderson and Uzumeri 1995, Loch et al., 2006). This strategy is based on singular challenge exploration for identified market and its potential reuse as a platform core after (Singular challenge platform emergence strategy). In addition, there is a possibility to develop first the technology that is more critical (technological lock-in) for several identified markets (platform derivatives) (Kokshagina et al. 2012). The strategy leads to identify common challenge relevant to several market derivatives and design common core that addresses all these alternatives (common challenge strategy).

The history of innovation promotes mostly singular challenge strategy to guide innovative development. But in certain sectors, like semiconductors, telecommunications, pharmaceuticals, the success of common challenge strategy applicable to several markets is more important than singular project success. Thus, which strategy to choose for innovative technological platform emergence?

Why common challenge strategy appears to be so challenging and risky? Why do we usually prefer to conduct project exploration to emerge common core instead of constructing future platform directly? What are the obstacles and advantages of each strategy? And which conditions one has to follow to choose one of them in organizing exploration activity? The objective of the paper is to define what are the precise market and technological conditions that in certain situations lead to 1) develop common building block (common core) that

facilitate all the others projects but don't provide access directly to the market 2) launch singular project exploration to emerge future platform core consequently.

Methodology. We attempt to address our research questions by formally describing each strategy and fabricating simple economical model to compare them. For simulation the data was created by taking into account specifics of real management situations and parameters were chosen based on the literature review. Then we illustrate the insights of the model through a case study of innovative technology development in semiconductor industry. The in-depth empirical case study was conducted in STMicroelectronics (STM), one of the leaders of the semiconductor industry. The data for case study was gathered from advanced technology platform with several interdependent modules developed by company and introduced to the several markets after all. The source of information was regular and frequent semi-structured interviews with specialists participating or leading technology development from R&D and business units and projects documentation.

Results and managerial implications.
This paper contributes to existing work on platform emergence by introducing the strategy of platform core construction in double unknown based on future common challenge investigation. To investigate the question of market and technology conditions of each strategy implementation, we formulate a model to formally describe risk management strategies for platform emergence. Thanks to this model we introduce technology and market compatibility rules that highlight the situations when common challenge strategy is possible. We illustrate both strategies on the case of advanced technology development in semiconductor industry and highlight the limits of each strategy. We show that the common challenge strategy aims at knowledge creation, attempts to keep cost under control of R&D budget, while maximizing the likelihood of being relevant for future markets. In addition, our study shows that the strategy of singular challenge project development for platform core emergence depends on the context of the first project context. There is a negative effect that the results of project exploration can fail to decrease uncertainties relevant to other options.

This work suggests managerial implications of strengthening the collaboration in between Technology R&D and markets divisions. This collaboration will allow the construction of flexible common challenges and enhance the value of common core relative to future market derivatives. However, the common challenge emergence has to be done in the earlier phases of technology exploration and markets emergence (breakthrough innovations) to avoid too many constraints added by each market derivatives.

Finally, using originally new way of risk management based on knowledge gap identification to construct common unknown core, company can build its innovative capabilities through knowledge management and better position to innovate in emerging fields.

Introduction

Innovative technological development requires capital investment in its R&D. Yet, the R&D outcomes normally take years to be realized and the economic return is highly uncertain. In this uncertain environment project selection, choice of the key technologies for the firm in upcoming years, the management of R&D are of crucial importance for high-tech companies. Therefore a lot of scholars and practitioners seek for tools to allow mitigating uncertainties and maximizing profit of future products in these breakthrough situations. The existing literature indicates the interest in developing technological platform to enable creation of products and processes that support present and future development of multiple modules (Gawer 2009, Baldwin, 2008, Baldwin and Clark 1997, Gawer and Cusumano 2008, etc.)). This strategy appears to be robust for new product development, manufacturing and distribution. According to (Sanchez 1996) concepts of modularity in products and organizations are the core concepts driving the new kinds of product strategies emerging in dynamic product markets. Historical stories of IBM technological platform development (Gawer and Cusumano 2008), Black&Decker power tools (Meyer and Utterback 1993) and many other examples show the relevance of platform strategies in practise. Usually, the platform is considered to be known and the development is based on exploiting this common core to build future modules. However, when we are in double unknown situations, markets and technologies are highly uncertain and neither options, nor platform core are known. There is a possibility to decrease uncertainties by launching exploration projects for identified potential market and manage risks at the level of single projects. But the preference of high-tech industries like pharmaceuticals, semiconductors is to develop technologies relevant to several markets. Thus, how can one ensure platform emergence in double unknown?

The first identified strategy consists in progressively emerging the common core. Generally this leads first to conduct exploration projects to create new knowledge and formulate platform. Second, reuse identified platform to construct platform derivatives (series of projects generate the technology platform with associated derivates) (Sanderson and Uzumeri 1995). This strategy has to take into account risks of the chosen exploration project at the beginning and consequently risks of portfolio once it is constructed. Finally, there could be a possibility to develop first the technology that is more critical (technological lock-in) for several markets (platform derivatives) and then address the options. This strategy doesn't obtain common core as a result of exploration, but directly working with common challenge as an object to conceive and manage (Kokshagina et al. 2012).

So platform emergence can lead first to exploration project emergence to fabricate platform or direct platform emergence to identified options. In the first strategy, at the beginning singular project is launched to find a solution to common singular challenge and after to formulate the common core for future options. The project is explored first and then the common to several market options is deduced based on it. In the second strategy, we formulate the common by identifying what is common to several options before trying any of them. But in the second case the common is not necessary an existing one – it is common challenge to several potential markets. We will call the first strategy "Singular Challenge" (S_P) and the second – "Common challenge" (S_C). Previous work described these strategies based on identified managerial framework with objects, actors, criteria and necessary resources (Kokshagina et al., 2012). Still, even if highlighting major differences in between strategies, their economical conditions are not explicitly addressed. It is not obvious which strategy to choose in each situation (research gap).

The history of innovation promotes mostly singular challenge strategy to guide innovative development. But in certain sectors like semiconductors we state that the common challenge is more important than singular project success. There are companies like Intel, STMicroelectronics, etc. that were able to propose solutions for common challenges before having developed any markets and then introducing products based on this common core to all the identified markets.

Thus, why common challenge strategy to appears to be so challenging? Why do we usually prefer to conduct project exploration to emerge common core instead of constructing future common ground directly? What are the obstacles and advantages of each strategy? And which conditions one has to follow to choose one of them in organizing exploration activity? The objective of the paper is to define what are the precise market and technological conditions that in certain situations lead to 1) develop common building block (common core) that makes accessible all the others options but don't provide access directly to the market 2) launch singular project exploration to emerge future platform core.

Generally for common challenge strategy there are a lot of constraints and it is considered to have little chance to succeed. Among them:

1. Capability to identify potential markets without adding too much constraints to the common core formulation
2. High cost of adaptation for future derivatives
3. Competence to identify common challenge in high uncertainty.

As all the markets are quite contrasted (different domains, criteria, etc.), usually there are high costs of adaptation for each option. Thus, we have a tendency to look for platform that reunites maximum functions in between different options and probability of finding common core becomes almost zero. By adding specific market requirements we increase the constraints and the risk of failure to find common challenge. It is shown that even formulated theoretically, common challenge strategy appears to be quite risky and difficult to succeed and requires particular expertise in both technology and marketing. Besides, the more classical strategy that leads to project exploration to formulate platform core appears to be risky as well. First of all, high level of uncertainty in the phase of project selection (subjective probability of success that changes over time), can lead to develop project that will not be suitable for being platform core. Second, it is not certain that the knowledge created during the project exploration can be reused in the phase of derivatives development. The strategy of project has a tendency to reduce the exploration space by formulating the context. Thus, we attempt to compare these two strategies by first, formally describing each strategy and fabricating simple economical model to compare these strategies. We will evaluate our model based on the exploration of innovative technology development in STMicroelectronics.

The paper is organized as following. First, we present existing risk management strategies based on literature review and define strategies suitable for platform emergence management in double unknown situations. Second, we present chosen research methodology. Third, using proposed framework, we formally describe each strategy to be able to compare them and highlight the differences. We create a simulation model that leads us to define the principles of technical compatibility and markets compatibility to enable the choice in between strategies. We describe briefly the context of empirical study and we illustrate singular project and common challenge strategies using the chosen case study of advanced technology platform development. Finally, the paper closes with managerial implications of platform emergence strategies and directions for further research.

Literature review and Problem Formulation

In this paper we deal with the management strategies in high uncertainty environment. Uncertainty is defined as a condition when decision maker doesn't know all the alternatives, there are risks associated with each alternatives or the consequence (Griffin, 2004). Under uncertainty little is known related to the list of alternatives or the outcomes, the probabilities associated with the state of nature are unknown (Agbadudu 1996). The economic success is influenced by many uncertain exogenous and endogenous factors. The future profitability depends as well on how the decision makers manage the projects after they start and it is well demonstrated in the literature that continuous uncertainty management is required for innovative projects (Loch et al., 2006). Literature review on risk management allowed identifying two types of strategies, which have a tendency to manage uncertainties at the level of projects or portfolios (platforms).

(Sanchez et al., 2009) showed that project risk management is a well developed domain in comparison to the program risk management and portfolio risk management fields. They stated that for portfolio management it is hard to find particular written methodologies. In portfolios usually we pilot risks case by case without considering influence of project dependencies in overall portfolio performance. The risk management methods based on uncertainty reduction for identified projects are well represented (a lot of work deals with studies on how decision makers cope with uncertainties (i.e., Lipchitz and Strauss, 1997; Chapman, 1990; projects with variations and foreseeable uncertainties in De Meyer et al., 2002), etc.). Risk management includes techniques to either increase probability of occurrence of an event or increase its impact on the project (or decrease in case of negative risks) (Petit, 2011). These strategies lead to minimize unknown by selecting a priori the less uncertain projects with higher probability of occurrence, depending on the identified market risks and technological risk. The level of uncertainty allows prioritizing corresponding markets (based on market probability) and selects a project associated with maximal economic performance (i.e., Expected NPV, Discounted Cash Flow). The risk management is concentrated on addressing uncertainties associated with project feasibility, market, technology, financial aspects, organizational, etc. (Ward and Chapman, 2003). On the level of project there is a tradition of uncertainty diagnosis and risk reduction for pre-defined problem (S_1). S_1 lead to minimize unknown by selecting a priori the less uncertain projects, depending on the identified market risks and technological risk. The criterion of "good" risk management is the high probability of success of the project. These strategies deal with projects independently and do not consider existence of common knowledge.

Risk management strategies in portfolio (S_2) try to take into account common aspects in between projects. The example of this is a portfolio represented by a technological platform core and its derivatives. This second family of strategies take advantages of interdependencies in between projects. For instance, in case of modularization (Baldwin and Clark 1997, 2004) propose to reuse the platform core that helps to address various options that are depending on it. (Baldwin and Clark, 2004) showed how to obtain several available options thanks to common platform. Platforms represent a core of technological system and have to be interdependent with other parts of the system (Gawer and Cusumano, 2008). According to platforms typology (Gawer 2010), we deal with internal, inside firm platforms in this paper. Reusing platform core attempts to minimize risks by constructing several options (Baldwin and Clark, 2004) (S_2). The module considered to be defined once the market signal is sufficient enough to conceive it (Baldwin 2008, Baldwin and Clark, 1997, O'Connor et al., 2008, Gawer and Cusumano, 2008, etc.)) (S2). This strategy increases chances to succeed not by selecting one single, most probable project but by increasing the size of the sample, i.e. by being able to play several options, maximizing the total economic value of the portfolio of derivatives. Risks are managed by the portfolio manager or the platform manager. The criterion of “good” risk management is the aggregated profitability of the portfolio (or platform). This strategy works on common, but it is limited to common already known aspects in between projects.

For more information about theoretical description of these two strategies see (Kokshagina et al., 2012). As was shown in the paper when it comes to highly uncertain situations (breakthrough, radical, disruptive, major innovation, etc.), S_1 might be impossible, because all projects are too risky. One still could make hypothesis (in case of unforeseeable uncertainty (De Meyer et al, 2002)) based on subjective probabilities of success, but they change significantly at the end of projects. S_2 strategy might be impossible because there is no platform available to play several times with limited costs in high uncertainty. In addition, existing literature on product platforms assumes that the platform leader knows the final use of products and is capable to develop these new products (Gawer, 2010). This is definitely not the case in the context of radical innovation when both the selection of platform core and final products use are highly uncertain.

A lot of researchers propose the way to deal with high uncertainty (“unk unks”, unforeseeable uncertainty, etc.). (Loch et al., 2008) provide an overview of existing strategies to unknown management. By showing that traditional risk planning techniques are insufficient for management of unforeseeable uncertainty, they suggested that the final method depends on the presence of unforeseeable uncertainty and complexity of the problem. Their work proposed a complementary model for diagnosis of unforeseeable uncertainty by learning problem structure and decomposing the problem. This research summarized two fundamental approaches for management of uncertainty:

- Trial and learning approach (Pitch et al. 2002, Van de Ven et al. 1999; Lynn et al. 1998) that consists in iterative trying of selected trials and flexible changes in the course of action
- Selectionism (Lenfle, 2011, Pitch et al., 2002, McGrath, 2001) consists of launching multiple trials in parallel and then selecting the best approach later. Selectionism is often considered to be more expensive and is affordable to use for big problems. Usually selectionism is less time consuming than trial and learning and more suitable for market driven approaches that need faster response.

In the situation of high uncertainty and low complexity they suggested to use trial and learning approach and in both high uncertainty and complexity - combination of trial and learning and selectionism (Loch et al., 2008). In (Kokshagina et al. 2012) is shown that both these approaches lead to fabricate trial project to create knowledge that attempts to reduce uncertainty and identify alternatives. This lead to risk management first at the level of project and consequently at the level of portfolio once the trial project results in platform core. So we acquire common core as a result of uncertain projects exploration that attempts to find a solution for a particular identified challenge. The developed common core serves as a basis of successive explorations. We call this strategy singular challenge platform emergence (S_p). Empirically (Loch et al. 2008) used combination of trial and learning and selectionism method of launching parallel trials in application to the case of Escend Technology start-up. (Sanderson and Uzumeri, 1995) showed how generational platforms were able to coexist within the Walkman product family consequently (project exploration) and support the development of important sub-families platform derivatives).

In (Kokshagina et al., 2012) the second type of strategy that deals with double unknown situations was introduced. This strategy doesn't attempt to obtain common core as a result of exploration, but works directly with "common challenge" as an object to conceive and manage (S_c). The common unknown is precisely the common challenge for several identified market options. In common challenge strategy there is a tendency to pay exploration phases that allows designing common core fabricated to emerging market derivatives. Common challenge strategy to pilot risks was introduced based on the literature review and its application was illustrated on empirical case of technology development in STMicroelectronics. This strategy doesn't deal with the same object, uses different resources and require particular conditions to its implementation. The actors responsible for its management have to take into account both technical challenges and emerging market needs and have a competence to connect them to create common core. Even if identified based on literature review, common challenge strategy is not widely used in practise since it is considered to be expensive, risky and hard to manage due to several reasons. Among them:

1. The capability to identify potential market derivatives in the case of double unknown. It is considered to be highly uncertain to already predefine the list emerging markets.
2. The capability to identify common challenge to several potential markets and construct common core.
3. The cost of adaptation of common unknown to address particular market options has to be relatively small to assure the profitability of the overall portfolio to justify preliminary investment for common core construction.

Thus, common challenge strategy creates particular condition for its implementation and in practice the tendency is to select less risky development of exploration projects type $S_{p.}$ Nevertheless, the more classical S_p strategy that leads to project exploration to formulate platform core appears to be risky as well. First of all, high level of uncertainty in the phase of project selection (subjective probability of success that changes over time), can lead to develop project that will not be suitable for being platform core and address already identified market. Studies of new industrial product failures consistently show that selection of inadequate market knowledge, brought about partially by ineffective market research, is a key contributor to the failure of project exploration (Maidique and Zirger 1984). Second, it is not certain that the knowledge created during the project exploration can be reused in the phase of derivatives development.

Therefore it is not evident which strategy is more advantageous for innovative technology platform development in double unknown. The objective of the paper is to precise what are the exact situations that make possible the identified strategies in double unknown, what are their limits and advantages. We attempt to define what are the precise market and technological conditions that in certain situations lead to develop common building block (common core) that facilitate all the others projects but don't provide access directly to the market.

To address our research questions, we attempt to define necessary criteria and resources to model each strategy, to characterize identified strategies and investigate applicability of risk management strategies in double unknown. Analytical model will allows us to understand the logic behind each strategy, its consistency and interest of each strategy in practice. We provide simplified model to compare identified strategies on an empirical case of BICMOSMW technological platform development in STMicroelectronics.

Methodology

The objective is to analyze the precise market and technological conditions that in certain situations lead to platform emergence based on singular challenge project exploration or common to several markets challenge identification. We propose a simple economical model to characterize identified risk management strategies. There are various ways to model risk management strategies behaviour. The goal for our model is not to explicitly model each strategy but to show in general the difference in between them, to show to which extend each strategy can be implemented in practise.

To explore the proposed model utility we carried out a field study in semiconductor silicon foundry and analyzed an empirical case of technological platform development to compare identified strategies. For this empirical study the primary source of data were regular and frequent semi-structured interviews. This work was conducted over 8 months period from (November 2010 – June 2011). This case of technology development implied portfolio of Collaborative R&D projects, PhD thesis, and business unit development projects. We organized interviews specialists participating or leading technology development from R&D technology and design groups, business divisions, former PhD students and some associated external research centres. Overall around 20 interviews were performed. The analysis was completed by the scope of documents as European projects reports, research presentations, and thesis manuscripts, database of thesis project descriptions. In addition, data analysis was followed by seminars with company managers (not necessary participating in technology development) to discuss the project, to test the validity of our hypothesis and enrich our propositions.

Comparative case study

We conduct our case study at STMicroelectronics, one of the leading semiconductor companies, in Advanced R&D research units that don't follow classical rules of R&D Management. The relevance of semiconductor industry for radical innovation studies was showed by various researchers (ex., Cohen, Levinthan, 1989), especially for knowledge creation methods in science-based environments (showed by Le Masson, et al. 2010, 2012) driven by "More Moore" Law (Moore, 1965). Strong competition, fast changing environment relevant to semiconductor industry lead it to explore not just new technologies, but as well new functionalities, creating new products. Advanced R&D units in STM don't follow "More Moore" law. They are subscribed in diversification approach that is identified by ITRS as "More than Moore" (ITRS 2007). There is neither clear scientific question, neither well defined decision to develop new products based on exploration and targeted markets. There is high level of uncertainty both on the level of technology and future markets.

To better understand the applicability of S_p and S_c strategies in double unknown platforms we illustrate their application on the case study of advanced technology platform development (Leguay et al., 2011). The studied case is BICMOSMW (high performance 0.13µm SiGe BiCMOS technology, targeting very high-frequency applications) technology platform development based on Heterojunction Bipolar transistor (HBT) with unique technology features. Despite of the difficulties in defining both future technology and designing market, the team succeeded to address several markets simultaneously. The choice of this case to compare both strategies is based on the differences identified through case studies interviews. First group of interviewers were presenting the case as management of exploration project of BISMOSMW technological brick development for radar application development and its reintegration for fast download and optical communication in the following steps. The experts who were responsible for the technology design and management showed that it was not executed completely neither at the level of project based strategy, nor at the level of portfolio based. The team leader based the platform exploration in addressing what was unknown for all the targeted markets; he was seeking for common challenge. The dependences were constructed based on common challenge identification (S_c) and were managed based on the links to allow exploration. These differences in case treatment lead us to apply our analytical model using both strategies to BICMOSMW case of technology development. We will further describe the case, identify which logic the technology development followed and compare performance of different strategies.

Framework introduction

To propose analytical model for project driven singular challenge S_p or common challenge strategy S_C, we start by characterising the classical S_1 and S_2 strategies. Risk management strategies deal with both market and technology uncertainty (Abernathy and Clark 1985). Market uncertainty is based on the volatility of market size, customer needs. Technology uncertainty concerns the volatility of available knowledge and field set (Oriani and Sobrero 2001).

Risk minimization at the level of projects – S_1

These strategies lead to minimize unknown by selecting a priori the less uncertain dominant projects from the list of identified candidates P_1,..P_i, depending on the identified market risks and technological risk. The level of uncertainty allows prioritizing corresponding markets (there is probability distribution for list of markets): $M_{1,\ldots,}\, M_n$ - list of predefined markets

$P_{1,\ldots,}\, P_n$ - list of corresponding probability of success for each market accordingly: $P_1(M_1)_{,\ldots,}\, P_n$ (M_n). Marketing is able to prioritize market and predefine a dominant market to address with associated project definition: functions; targeted clients, technical specification.

Cost of the projects exploration: $C_{1,\ldots,}\, C_{n.}$ Cost of exploration of each project is high and with defined budget one can pay only one project. We consider that the cost of projects exploration is limited by predefined budget of R&D (B). So $C_{1,\ldots,}\, C_n \sim B$. The budget of a project is predefined (usually R&D budget for project development) and we consider that selected project P_i will not overcome it. Π is expected profit.

The expected benefits are determined on the basis of estimated a priori subjective probability function. The probabilities are regarded as being subjective which helps to avoid restriction in business decision making (Joseph 2010). The expected value is the weighted average and is found by multiplying each expected net present value (*ENPV)* by its respective probability. The decision maker has to select the dominant project from different candidates that can have similar or different net present value and with different probability distributions (by using a normalized measure of a dispersion of probability distribution). Therefore, one has to weight the alternatives and determine the one more feasible.

We select project *i* based on prioritized candidates list based on the ratio of the standard deviation and mean of expected value. The expected value of each project is calculated based on: $NPV_{S_1} = \max P(M_i)\cdot \Pi(M_i) - C_i$, where $C_i \le B$.

The criterion of "good" risk management is the high probability of success of the project and minimum coefficient of variation for project with controlled budget. The coefficient of variation (standard deviation divided by expected value) is used to measure risks while comparing alternative projects. The higher the coefficient of variation, the riskier is the project. The risk management is concentrated on addressing uncertainties associated with project feasibility, market, etc. Risks in S_1 should be managed by the project leaders that are capable to define and calculate information based on probability of success of different solutions, to reason based on both technical and market planning. The resources needed for project risk management is information based on functionality of the project, future users, and technical principles.

Risk minimization at the level of portfolio – S_2

Risk minimization at the level of portfolio consists in using an existing platform core (minimal system) to construct several options (S_2). This strategy increases chances to succeed not by selecting one single, most probable project but by increasing the size of the sample, i.e. by being able to play several options, maximizing the total economic value of the portfolio of derivatives. Risks are managed by the portfolio or platform manager. The criterion of "good" risk management is the aggregated profitability of the portfolio (or platform).

In platform driven strategy, platform core is considered to be predefined - P_{of}. In (Baldwin and Clark, 2004) the total economic value of the system is expressed as the sum of minimal given value (platform core in S_2) plus the incremental value added by each module.

The objective is to construct market derivatives that are based on P_{of}. Strategy responsible has to select a platform core from the list of given already (identified) potentially candidates. In the case of S_2 we can have several cores and take the one that aggregates better: the choice in between platforms.

There exist a list of modules with equal rather low probability (not possible to select dominant project), so one can play several options. Predefine probabilities of market derivatives are normally low and therefore they are not interesting for S1 strategy $P_{1,\dots,}P_n$ <1 (low probability equal for different derivatives; where n – number of project derivatives) that are derivatives of our platform (can't make a prioritized list) with associated market derivatives: $M_{1,\dots,} M_n$.

Aggregated cost value of market derivatives development has to be slightly low and reuse maximally already existing platform core. Each option attempts to address different market derivative maximizing the total economic value of the portfolio of derivatives. The constraint is defined by budget: $\Sigma C_i \sim B$

The Expected value of the system is the expected aggregated profitability of the portfolio (or platform).

$$ENPV_{S_2} = \sum_i NPV(P_i) = \sum_i (P(M_i) \cdot \Pi(M_i) - C_i) = n \cdot p \cdot \Pi - B$$

We suggest than in the case of low probability of n market derivatives, we can consider them more or less equal: $\sim p$. Portfolio manager has to know well the platform to identify derivatives. He has to manage the portfolio of options and probability that the set of chosen options is profitable. The information needed for platform driven strategy is based on platform core and cost of options.

As was stated before in the situation of radical innovations one can't predefine dominant market in S_1 and it is impossible to predefine platform in S_2 to address platform derivatives. The literature leads to identify S_P strategy that starts as project driven and then attempts to reuse identified projects as a platform core.

S_P: Singular challenge – Platform emergence strategy

Traditional risk management criteria like (Discounted Cash Flow, Internal Rate of Return, Cost-Benefit Ratio, etc.) assume the predefined value of uncertainty over the course of projects development, and thus, don't consider flexibility necessary for projects in double unknown. In the situation of radical innovation we can't select a market with higher probability and implement S_1 strategy. As well, we don't have platform core to address all these markets with equally flat probabilities distribution.

When the payoffs (future benefits) and costs are uncertain, one will have chance node that reflect two or more probabilistic payoffs and cost scenarios. This implies that the distribution of costs (C_i) and future benefits will have a non-zero variance. To reflect managerial flexibility, one could embody decision nodes that enable management to eliminate poor alternatives to avoid bad outcomes. The uncertainty affects the expected investments values by widening the distributions (Benaroch, 2002). Thus, the market probability distribution in the high uncertainty situations are considered almost flat and in the first phase of S_P is hard to prioritize markets. We consider that it is still possible to prescribe subjective probabilities in order not to select dominant market but estimate which of them can be less risky and more accessible for future derivates in options construction after. Therefore we select less risky project based on coefficient of variation of expected project value: $ENPV_{S_p} = \max P(M_i) \cdot \Pi(M_i) - C_i$, where $P(M_i)_{S_p} \leq P(M_i)_{S_1}$

It is important to mention that at the beginning of S_p we are not able to identify future common core for portfolio construction. The project exploration results in platform core construction. We complete this strategy by the second phase of options exploration with identified market derivatives: $M'_{1,\dots,} M'_n$. Project P_1 developed in the first phase is becoming a platform core $P_1 = P_{0PF}$ that makes accessible low probability markets in the second phase. The knowledge created in the first phase attempts to decrease uncertainty relevant to other projects. The total expected value of this strategy is the sum of the expected value of the first exploration project and aggregated expected value of project derivatives.

$$ENPV_{S_p} = \sum_i (P(M_i) \cdot \Pi(M_i) - C_i),$$

where aggregated value contains the value of both exploration project and market derivatives. The risk management in this combined strategy depends mostly on good exploration of project in the first phase. Platform core selection is limited to first developed project and its flexibility for identified future options. In S_P we assume several risks: we accumulate uncertainty relative to project selection and developing platform core and risks associated to derivatives management. In addition, there is a risk that the chosen project will not result in the platform accessible to market options. For project manager in S_P there are the same risks as in S_1, just it is much more challenging to identify exploration project in high uncertainty. Regarding cost of exploration, there is a high uncertainty in budget required for the second phase based on how well the exploration was identified and managed. If project gives a platform accessible for already identified options, portfolio manager has competence to pilot proposed platform core and associate it with valuable options, otherwise the necessity to adapt system could lead to expensive development.

S_C: common challenge

The major difference of S_C in comparison with other strategies is that we deal not with existing object but with a concept of common challenge. Instead of starting with project exploration to reuse maximum known, we start directly with platform identification as common challenge to several market derivatives. As we saw in previous research there are situations (in emerging fields), when it is possible to identify dependencies in between emerging future markets and construct common core in between them. These dependencies are based on market and technology to identify common challenge. We characterize market interdependencies as common functional characteristics for set of various markets.

The logic of common challenge is to fabricate common core for the list of potential market candidates with low probability. We consider the set of potential markets $M_{1,..,}$ M_n. Then common challenge is a technological building block to addresses certain specific functions common to the set of identified markets.

The risk management criteria based on uncertainty reduction (max value with min deviation) is not explicit for common core strategy. We are dealing with exploration space where it is impossible to highlight probabilities for markets and technologies that don't exist yet. Instead, we are increasing the variety of options to play. We construct common core based on common dependent elements between emerging markets. These dependent elements are common functional challenges for set of various markets. In this strategy we launch a preliminary phase of common challenge identification that will address all the identified options. There is a preliminary cost of paying P_O projects that significantly reduce the cost of each option development.

We maximize the variability of options to play later. By introducing S_C strategy, we found out that for good risk management in double unknown it is necessary not just to minimize uncertainties for selected exploration space, but also to maximize the variability of future options. There is certainly risk associated to common challenge identification. The total expected value of the platform developed:

$$ENPV_{S_C} = \sum_i (P(M_i) \cdot \Pi(M_i) - C_i), \text{ where } \sum_i C_i + C_{P_0} \leq B$$

The aggregated cost of development of all the alternatives and cost of common challenge exploration should be inferior to budget of R&D. Further we illustrate singular challenge S_p and common challenge S_c strategies using empirical material to highlight the major differences, specifics and prerequisites to launch these strategies.

Empirical Data

Case description BiCMOSMW platform development

We illustrate the identified risk management strategies on BiCMOSMW (high performance 0.13µm SiGe BiCMOS technology, targeting very high-frequency applications) technology platform development (Chantre et al., 2010). (Chevalier, 2007) showed that high-speed BiCMOS roadmap is driven, on one hand by the increase of the optical communications data rate, and on the other hand by the emergence of applications at higher frequencies. It doesn't follow classical More Moore law. Si/SiGeC heterogeneous bipolar transistor (HBT) performances can be pushed forward (with significant advantages over CMOS) and applications at ever increasing frequencies carry on.

In STMicroelectronics, BISMOSMW platform has evolved after several generations of technical solutions. Started with BiPx project it leads to BICMOSMW (specifically designed to address emerging millimeter-wave applications) and beyond. The history of bipolar transistor technology based on SiGe in STMicroelectronics started in 1998 with 0.35 µm technology for wireless communication (Geynet, 2008). The success of the SiGe HBT has come from its compatibility with silicon technology allowing both low-cost and high yield. While bipolar-only technologies are attractive to replace III-V technologies; full benefit is obtained by using heterogeneous solution of BiCMOS + CMOS devices. In spite of the ever-increasing constraints brought about by integration with CMOS (thermal budget, structural issues, etc.), HBT performance was dramatically increased over the past 10 years.

Till 2002 the group was working on optimization of bipolar transistor for analog signal processing to address emerging standard of 60GHz. There was no particular client demand at the beginning. The technological basis that was developed was not ready to address any market that time and the key technology was based on CMOS. In the following 2002 the group was analyzing which potential high-volume market and technological effort needed to develop to address it while reusing the previous research results on bipolar transistor.

The expert (Technology Line Manager) that initiated technology development was looking for an emerging market with potentially huge volume to assure return on investment. He identified a particular system issue: the Wi-Fi connections in the big public systems like airports, train stations, and more generally high-density places with a lot of connectivity devices. The current issue was with the standard for Wi-Fi communication (2.5 to 5 GHz), the frequency of processing information was too low to ensure connectivity substantial debit to each device. Thus, one potential solution was to use a 60GHz Wi-Fi system with a long range (>10m) to limit the number of base stations and system complexity. Functional requirements of the identified system contained both high-frequency emissions that were addresses by developed BICMOSMW technology that helped to combine different functional requirements as low power consumption, digital signal treatment, covered distance, etc. They used this knowledge to address automotive radar, optical communications, wireless fast download systems, high speed instrumentation, non invasive imaging and standard linear products identified market derivatives. In the following we attempt to use this briefly described case to show the potential outcomes of technology developed treated either by project singular challenge strategy or common challenge strategy.

Illustration with case study

We consider that each market follows normal probability distribution (evaluation of mean and standard value) to show the evolution of market probability distribution in the course of technology development. At the beginning in double unknown all the distribution curves are flat. To simplify the calculation we use discrete probabilities P_i and we suppose that all the market have the same volume *V*.

Initially 6 potential markets were identified with associated probability distribution for BISMOSMW. As uncertain emerging markets in this paper we consider existing markets with added new functionalities (new products) or completely new markets: M_1: radar (automotive market); M_2 : optical communication; M_3 : standard linear products; M_4: medical ; M_5 : hard disk drive applications; M_6 : fast download (figure 1, each project addressed each market accordingly).

We suppose the cost of development for technology for each market is equal to C_i $(C_i=150)$. Here the cost of development is the cost of technological building block development only. At the level of market division there is no difference in between BICMOSMW technology prototype for radar application or for Wi-Fi for airport since both technologies are using similar phenomena. The tests, package types, interconnections, mode of implementation, etc. are always necessary for final product development and they don't vary significantly with the features of technical core. So we don't take these costs into account (they are inevitable for product development). The defined budget of R&D for technology platform development is $B=250$.

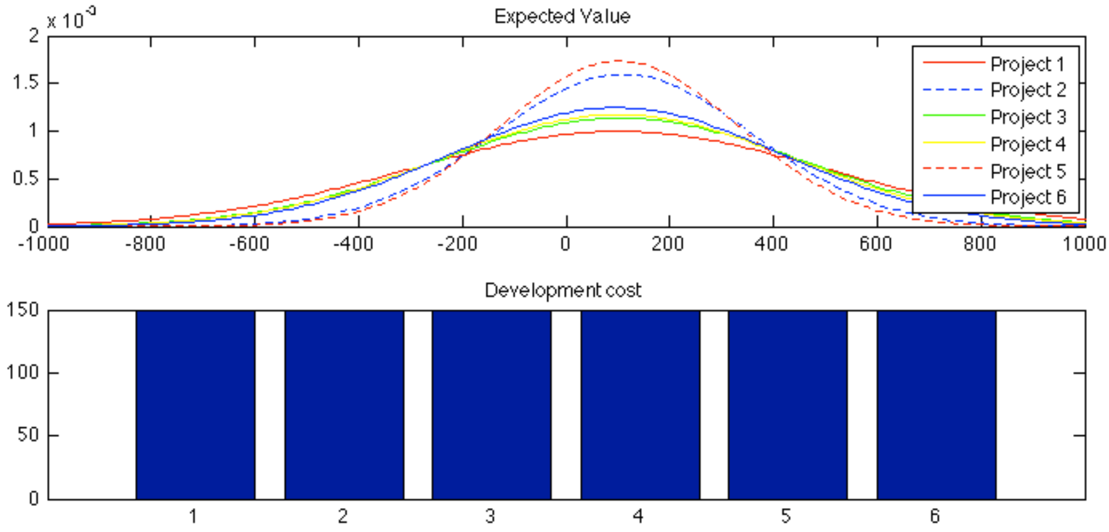

Figure 1 Initial situation for identified projects (Expected value distribution and Development cost for identified projects)

Singular challenge – Platform emergence strategy S_p

As we are in the situation of high uncertainty all the markets have really low probability and high volatility (figure 1). The markets probability distributions are almost identical. Therefore, it is hard to select one most promising project. Nevertheless, market analysis showed that the radar application was more certain and the team start to explore potential bipolar technology targeting automotive market. By decomposing the projects, they defined functional requirements (FRs) relatively to the automotive market: low consumption, high frequency, cover long distance, integrated solution (CMOS+bipolar). The cost to develop first project was determined as $C_1 = 150$. We consider that in the course of development, there is a chance that the project launched will minimize the cost of development of other dependent projects. We fabricate common core (figure 2). Once the technology developed, there are two scenarios possible.

Scenario 1. Technology developed in the first phase of S_p allows decreasing uncertainty relatively to other projects.

For example, high frequency of operation and low consumption of technology developed for automotive industry are relevant to optical communication and fast-download system as well. So the exploration project decreases significantly uncertainty relevant to the 2 and 6 projects. The challenge is that the cost of development for identified derivatives 2 and 6 should be $\sum C_i \leq B - C_{P_0}$. With the $B = 250$ and $C_1=150$. The $C_2+C_3=100$. For the budget that rests, one can develop derivatives project 2 and 6. The existing bipolar solution was not able to achieve desired for optical communication speed 100GB/s. For project 2 of optical communication the new function relatively to already developed in the first phase of S_p was to ensure high voltage capability for optical communication and low noise. So the team had to redesign the system developed in S1' to adapt it to the new market with the cost of exploration C_2. For fast download (project 6) one had to ensure emerging 60GHz standard with assigned cost of adaptation – C_6. In the second phase, the situation described is presented in the (figure 3, left).

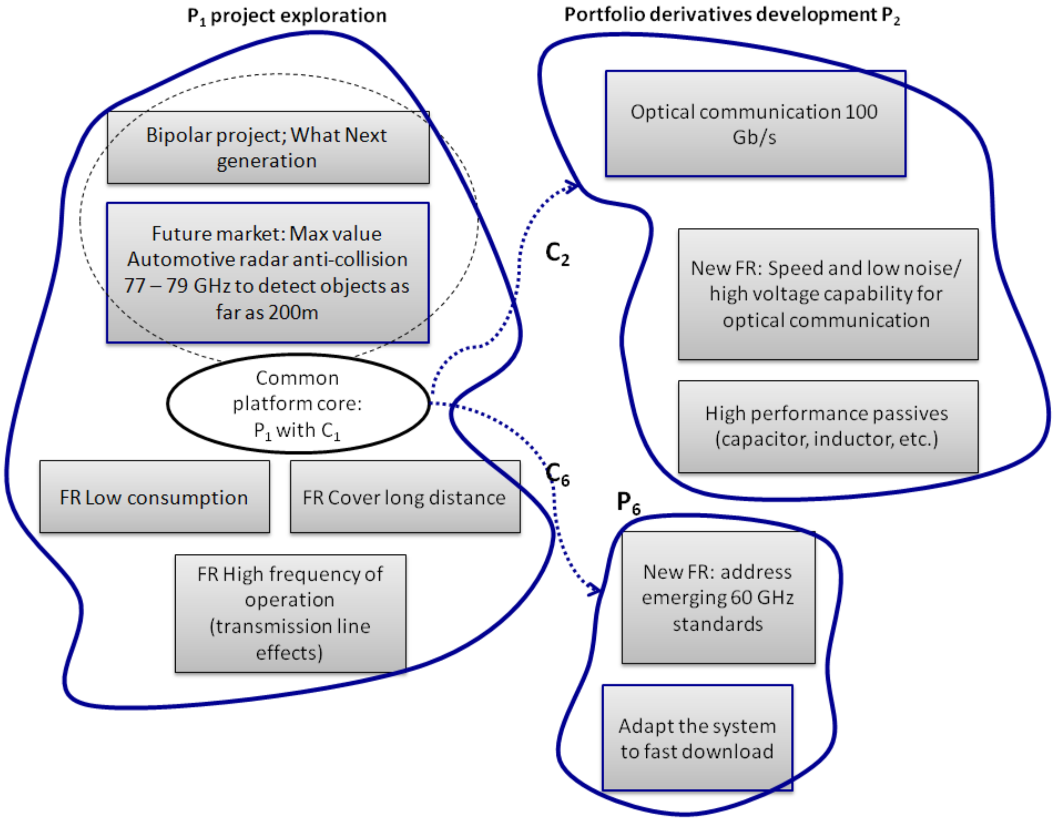

Figure 2 Technology platform developed followed by S_p strategy

Scenario 2. Technology developed in the first phase of S_p didn't allow decreasing uncertainty for other potential market derivatives.

This is the extreme case when technology explored for radar communication was specifically oriented to automotive industry. Therefore, the level of integration of developed technology and knowledge created during project exploration were not relevant to any of other options identified. In this case, the level of uncertainty relative to other options after radar application development doesn't change and therefore the cost of development remains the same. The project fails to form a platform core since the exploration space was restricted by problem setting in the first place. Project derivatives can't reuse knowledge created to decrease uncertainty and the cost of adaptation is still high (figure 3, on the right).

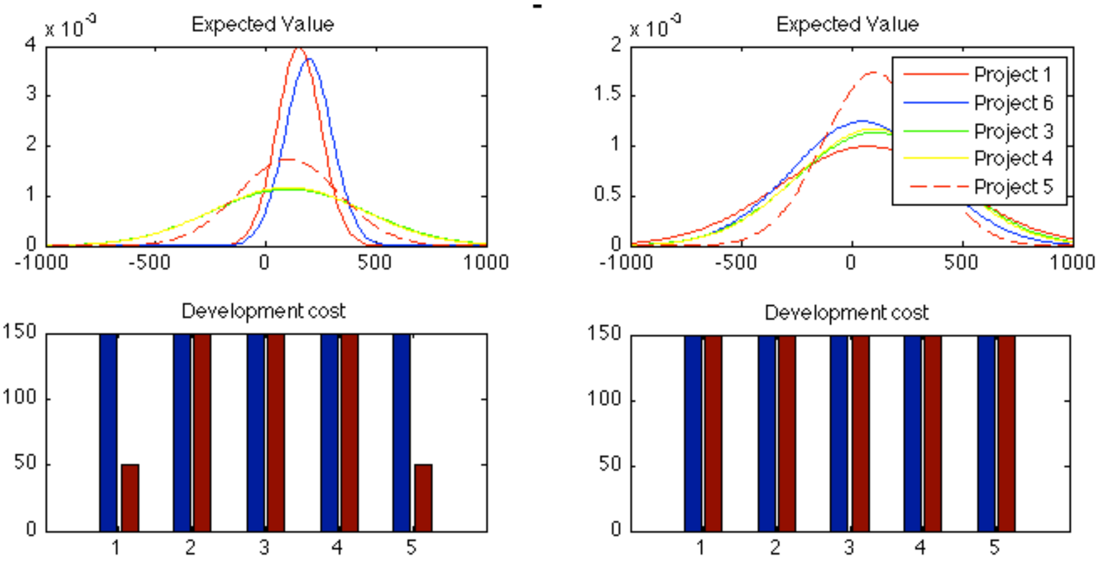

Figure 3. Second phase of S_p strategy after project exploration. 1. Expected value distribution and development cost for platform core (positive scenario) 2. Negative scenario (right).

Common challenge S_C

We consider the same identified markets as in S_p, the same costs at the beginning. But there is an expert that proposes a project of technological development common to all identified markets P_0. Common base to 6 markets in this case is precisely common challenge. But if identified P_0 works, we arrive to decrease the cost sufficiently. In this case there is a possibility that there is a common core which is not known a priori but its creation provokes significant minimization of the cost for other projects – common challenge. In this example common challenge is the Wi-Fi system at the airport. The technological phenomena developed addresses maximum functions with the particular budget associated to access all the defined markets. The developed technology BICMOSMW allowed to explore maximum functions with fixed budget of R&D and addresses several markets at the same time. **Common challenge** was identified as knowledge gap that would connect knowledge related to functions of the future system (figure 4).

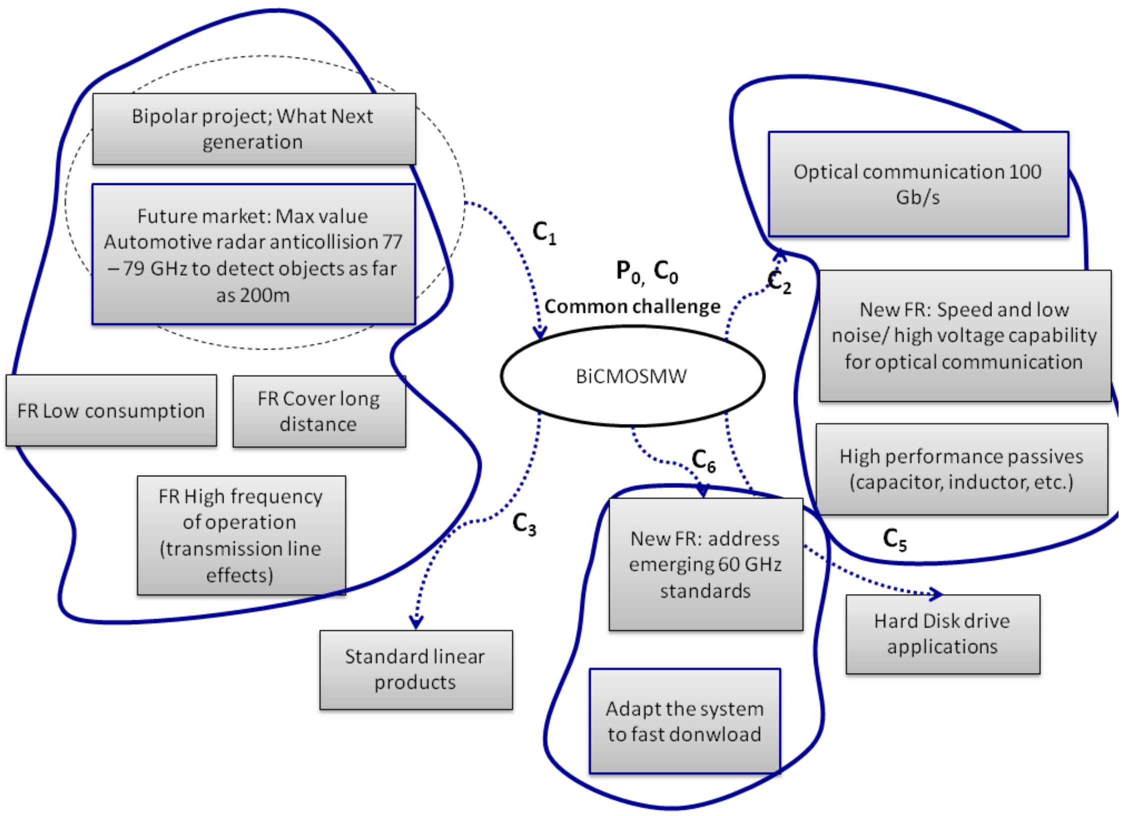

Figure 4 Technology platform development followed common challenge strategy S_c

For Common challenge strategy to work one needs to identify common challenge and invest in one technology that will decrease the cost of development for all identified markets. On the contrary to previous strategy, at the end of common core development the probability distribution doesn't change significantly but the cost of research decreases sufficiently for each market (figure 5). Suppose that $B = 250$. If the cost of development for P_0 project: $C_1=190$. $B-C_{P0}>=\Sigma C_{Pi.}$ By developing the common core that was precisely BICMOSMW for WIFI airport system, we decrease the cost sufficiently and develop other options with the rest of the budget (fig. below).

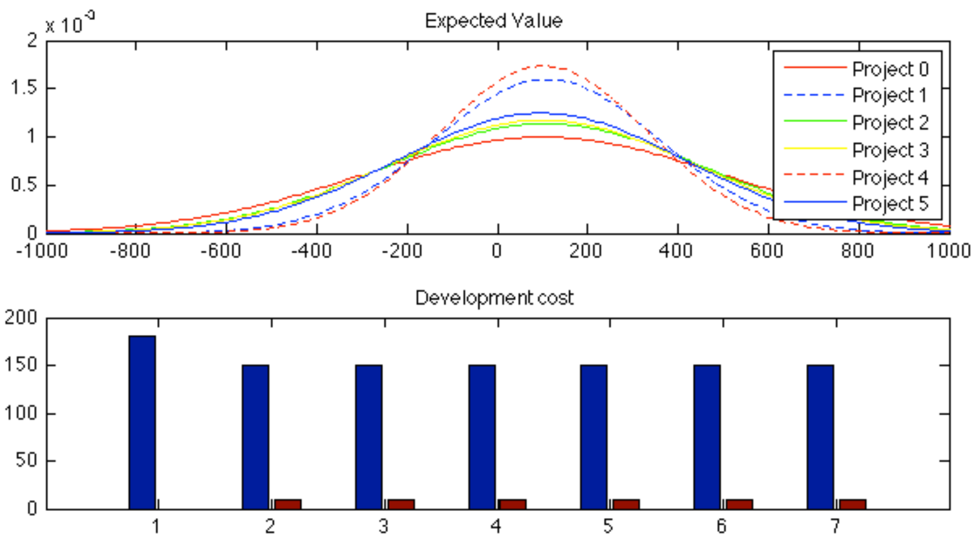

Figure 5 Expected value and development cost distribution after common core construction

Discussion

Based on the proposed simplified models we see that the success of both strategies depend on the first project selection in S_p and Common challenge S_C identification P_0 in the second strategy. Through the simple models:

1. We've noticed that it is necessary to compare the probability of success of technical development of common challenge to the probability of the first exploration project. Does the success of the strategies depend on the probability of P_{S1} and P_0?
2. We saw in the second strategy that the success of the strategy depends on the effect of project P_0 to the cost decrease for projects $P_1,..,P_6$. There is a gain on the cost of development thanks to P_0, but ad hoc we have to minimize risks. While launching common challenge strategy, we don't have to look for P_0 with minimum cost of development, but rather P_0 that allows developing all the modules with the cost inferior to the budget. So we need to find common challenge which ensure the aggregated cost of modules development and common challenge exploration is inferior to predefined budget of R&D.

We formulate the law of interdependency to construct common challenge for various alternatives. Normally the more alternatives we have, the less is probability of common challenge identification. It means that the probability to address N markets ~1/N. To construct common core we attempt to predefine future links in between different market functions. The complexity works on the number of links in between these functions. For example, for 2 markets we can have one link, for 4 markets – 6 links. We suppose the connections in between markets can be developed with likelihood costs. If we consider new market for common challenge strategy, we need to validate if it is compatible with all the others markets. Number of compatibilities to ensure in between different markets: for 2 markets – 1 link of compatibility, for 3 markets - 3, for 4 – 6 links, etc. For k markets, the probability of market k to be compatible with the others is equal to $P^{\frac{k(k+1)}{2}}$. The more alternatives we have the less is probability. In this case, one would never find common core in between several potential markets due to extremely low probability if existence.

Based on the case study, we suppose that the success of common core strategy doesn't depend on the number of identified markets. In the case of common unknown $p = P_0^* = const \ \ \forall k$. For instance, the probability that we find unique BISMOSMW for 6 markets or 2 markets is the same and equal to P_0^*. Thus, we formulate the law of market compatibility $P_0 = P_0^* \cdot p^{\frac{k(k+1)}{2}}$ where P_0^* common core probability and p – probability of all the links to ensure for k markets. Suppose the probability that we common core has a high level of compatible with the markets p~1 because technology that targets high frequency is used by all identified markets. In this case $p = P_0^* = const \ \ \forall k$. Thus, normally we don't launch common challenge strategy because probability to connect N markets is almost zero and cost of compatibility to ensure is considered to be high. We show that the common challenge is possible when compatibility of common core is high for all the identified markets. It means the probability of being capable to develop the links is high. So we formulate:

The law of market compatibility: the probability of common challenge existence doesn't depend on the number of alternatives; but on high probability of compatibility in between markets. $P_0 = P_0^* \cdot p^{\frac{k(k+1)}{2}}$, when p~1 $p = P_0^* = const \ \ \forall k$ for common challenge.

The second challenge for technology development in high uncertainty is to keep the development cost under budget constraints. We need to explain the structure of the cost. At the beginning of development there is an initial knowledge base K_0. In the S_p strategies the trajectories of projects development are independent at the beginning. When we develop first exploration project there is a possibility that we decrease the cost of development for other projects. Thus, in the second phase of S_p, trajectories (due to minimized uncertainties and knowledge reuse) can be shorter or don't change depending on the results of first phase exploration (figure 6). In the figure, the P_1 project launched in first phase decreases the cost of the 2nd and 3rd projects (trajectory in red).
While starting technology development, we don't usually look for P_0 project that is certainly longer then P_1 and cost of C_{P0} is superior to the cost of P_1, but we prefer to develop market that is certain to pay. However, once if we find P_0 project that allows decreasing uncertainties common to other projects, we are much closer to identified markets, trajectories for all the options are much shorter (figure 6). In this common challenge strategy there is preliminary cost C_{P0} for common core developing, but it allows obtaining the cost of adaptation ε that is significantly smaller than the cost of development of each of these alternatives C_i: $\sum (C_{P_1} + .. + C_{P_N}) \geq C_{P_0} + N \cdot \varepsilon$ (trajectory in green; figure 6).

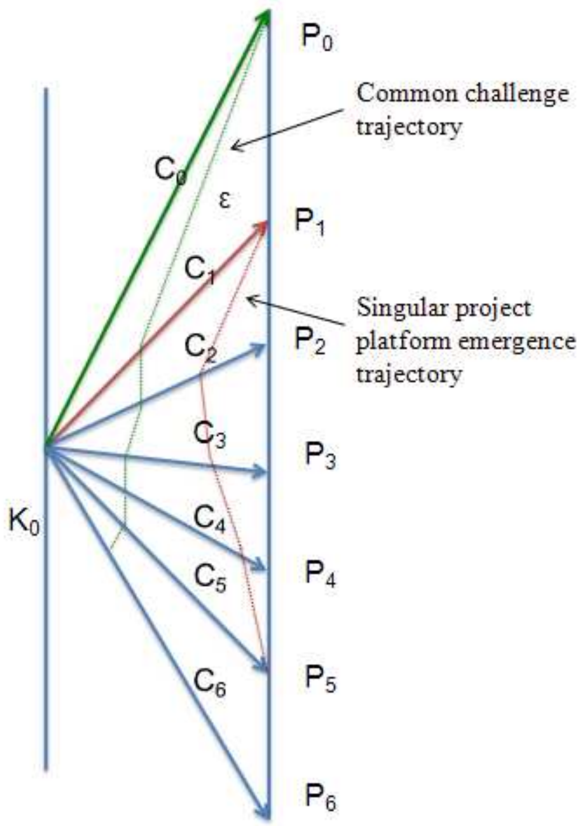

Figure 6 Project trajectories emergence

Thus, *we formulate the law of technology compatibility*: *one can pay technological common core development if it allows addressing all the identified options with the cost inferior to budget of R&D* $\sum (C_{P_1} + .. + C_{P_N}) \geq C_{P_0} + N \cdot \varepsilon$

How can we ensure costs of technology adaptation to be low? We can guarantee costs to be low if there is a high level of compatibility between markets and we attempt to share knowledge among them. Launching development project by project, we don't seek for connections in between these projects since there a lot of potential alternatives for each market and we can result in developing different technologies for each of the market. While developing one project, the purpose is to develop the technology that pays right away for identified market, and not the one that has potential to connect several ones.

Certainly on the level of mature project development and established markets, one would not look for technology and market compatibilities. However, in the early phases of technology investigation in double unknown, one could play potential market compatibilities with related technological functions. To common challenge strategy to function, there should be a lot of different applications, alternatives using similar technological phenomena. For example for the automobile industry, the number of alternatives is restricted to particular geographical regions, different economy, production is integrated and most probably common challenge strategy is impossible to exist. On the contrary, the enormous amount of technological alternatives and modularity in semiconductor industry makes common challenge strategy particularly interesting.

Results

This paper contributes to the literature in technological platform emergence in the double unknown situation when technology and markets are highly uncertain, neither platform core, nor options are known. We have compared two possible strategies of platform emergence in double unknown:

1. Project emergence first to find solution to the singular challenge market challenge and then formulate the platform based on this project to launch platform derivatives – "Singular challenge"
2. Platform emergence first to find solution to identified common challenge relative to potential options and then adapting design platform core to these market derivatives – "Common challenge". In this strategy common core doesn't exist at the beginning of the exploration.

This paper allowed determining the situations when common challenge strategy that is usually expensive, risky and hard to manage is attractive. We characterized what are the markets and technological conditions that in certain situations allowed developing technology core to support the others (common challenge) but don't access directly to the market. We showed that it is possible in certain conditions of cost structure and probability of market existence by introducing two laws of technology and market compatibility for common challenge strategy.

Law of market compatibility*: probability of common challenge existence P_0 doesn't depend on the number of alternatives; but on high probability of compatibility in between markets.*

$P_0 = P_0^* \cdot p^{\frac{k(k+1)}{2}}$, when p~1 $p = P_0^* = const \quad \forall k$ for common challenge, k – number of markets.

Law of technology compatibility: *one pays the technological common core development C_{P0} if it allows addressing all the identified options $P_1,..,P_N$ with the aggregated cost C_{Pi} inferior to budget of R&D* $\sum(C_{P_1} + .. + C_{P_N}) \geq C_{P_0} + N \cdot \varepsilon$, where ε – the decreased cost of market options developed after common core exploration.

While guarantee these two conditions, one can rely on common challenge strategy to exist when all the markets have equally flat probability of distribution in double unknown. If one of the markets is potentially better and there are dependent markets that potentially can reuse the technology developed to this singular market, the singular challenge platform emergence strategy is more attractive.

Using the introduced rules of technology and market compatibility, one can test whether common challenge is applicable in each particular case. However, proposed framework is static. Introducing important strategies of risk management in highly uncertain situations and conditions if their implementation, we haven't yet taken into account dynamic in different sectors. The further work relative to technology transfers and market emergence will need to investigate which strategy one has to adopt according to the markets and technologies dynamics. The model will integrate the emergence of new markets in the form of new functions combination and modelling of existing and emerging technologies in the form of graph related entities.

In the proposed work, we didn't take into account the competition dynamics in between different markets and other industrial actors. More investigation is necessary to understand competition dynamics influence and we will attempt to take it into account further.

Conclusion and managerial implications

The proposed study explores a gap in the literature relevant to the choice of platform emergence strategy in double unknown situation in between: 1) consequent emergence of project relatively to singular market challenge and then creating a platform core that address market derivatives based on that project and 2) platform design based on common challenge identification relevant to several markets. To investigate the question of market and technology conditions of each strategy implementation, we formulate a model that synthesise the description of each risk management strategy for platform emergence. Thanks to this model we introduced technology and market compatibility rules that highlight the situations when common challenge strategy is possible. We illustrated both strategies on the case advanced technology development in semiconductor industry and highlighted their limits. The proposed model is static and its objective is to understand major differences in between strategies behaviour relative to development costs and market probabilities. Further research will need to sophisticate our model to take into account specifics of real situations (competition, stochastic evaluation, etc.). This model will need to take into account dynamic of platform renewal and technology and markets emergence in the course of time.

This paper contributes to existing work on platform emergence by introducing the strategy of platform core construction in double unknown based on future common challenge investigation. Based on the proposed framework, we introduced technology and market compatibility rules to formulate common challenge relevant to several markets considering the high probability of compatibility in between market derivatives and common challenge that decreases the cost of technology adaptation for identified options. Therefore, the common challenge strategy aims at knowledge creation, attempts to keep cost under control of R&D budget, while maximizing the likelihood of being relevant for future markets.

In addition, our study shows that the strategy of singular challenge first project development for platform core emergence (S_P) depends on the context of the first project setting. There is a negative effect that the results of project exploration can fail to decrease uncertainties relevant to other options.

As it has been shown, the common challenge strategy is relevant to the industries with different part of alternatives, mostly for upstream type of industries like semiconductor silicon foundries. Even if some academics and practitioners insist on independency of advanced research exploration from addressing concrete markets issues, our work suggest a managerial implications of strengthening the collaboration in between Technology R&D and markets divisions. This collaboration will allow the construction of flexible common challenges and enhance the value of common core relative to future market derivatives. However, the common challenge emergence has to be done in the earlier phases of technology exploration and market emergence (breakthrough innovations) to avoid too many constraints added by each market derivatives, because this will lead to rigid core construction and increase the cost of development.

Finally, using originally new way of risk management based on knowledge gap identification to construct common core, company can build its innovative capabilities through knowledge management and better position to innovate in emerging fields.

References.

Abernathy W. J. and K. B. Clark, (1985), Innovation: Mapping the Winds of Creative Destruction, *Research Policy*, 14(1): 3-22

Agbadudu, A.B. (1996). *Elementary Operations Research*, Vol.1. Benin City: Mudiaga Press.

Baldwin, Y. C. (2008). Where Do Transactions Come From? Modularity, Transactions, and the Boundaries of Firms. *Industrial and Corporate Change*, 17(1), 155-195.

Baldwin, Y. C., Clark B. K. (2004). Modularity in the design of complex engineering systems. Book chapter in Complex Engineering systems: Science meets technology (Ali Minai, Dan Braha and Yaneer Bar Yam, eds.) *New England Complex System Institute Series on Complexity, Springer-Verlag*, New York, NY, 2006.

Baldwin, Y. C. and Clark B. K. (1997). Managing in an Age of Modularity. *Harvard Business Review*, September-October 1997.

Benaroch M. (2008) Managing Information Technology Investment Risk: A Real Options Perspective . *Journal of Management Information Systems.*

Chapman, C. (1990). A Risk Engineering Approach to Project Risk Management. *International Journal of Project Management*, 8, 5-16.

Chapman, C. and Ward, S. (2003). *Project Risk Management – Process, Techniques and Insights* (2nd edition). John Wiley & Sons.

Chantre, A., Chevalier, P., Lacave, T., Avenier, G., Buczko, M., Campidelli, Y., Depoyan, L., Berthier, L. and Gacquière, C. (2010). Pushing conventional SiGe HBT Technology Towards "Dotfive" Terahertz. *EuMA*

Chevalier, P. (2007). High-Speed SiGe BiCMOS Technologies: 120-nm Status and End-of-Roadmap Challenges. *in proceedings SiRF*, 2007, 18 – 23

Cohen, M. W. and Levinthal, D. A. (1989). Innovation and Learning: The Two Faces of R&D. *The Economic Journal*, 99(397), 569-596.

De Meyer, A., Loch, H. C., and Pitch, T. M. (2002). Managing project uncertainty: From Variation to Chaos. *MIT Sloan Management Review*, 43(2), 60-67

Gawer, A. and Cusumano, M. A.(2008) How companies become platfrom leaders. *MIT Sloan Management Review*, 49(2), 28-35.

Gawer, A. (2009) *Platforms, Markets and Innovation*. Edward Elgar Publishing, Inc.

Gawer, A. (2010). Towards a general theory of technological platfroms. Paper to be presented at the *Summer Conference 2010 on "Opening Up innovation: Strategy, Organization and Technology" at Imperial College London Business School*, June 16-18, 2010.

Geynet, B. (2008). Developpement et etude de transistors bipolaires a heterojonctions Si/SiGe:C pour les technologies BiCMOS millimetriques. *PhD thesis*.

Griffin, R.W. (2004). *Management*. New Delhi: Houghton Mifflin Company
Eisenhardt, Kathleen (1989). Building Theories from Case Study Research. *The Academy of Management Review*, 14(4), 532-550.

Joseph, I. (2010) Project Decisions under Uncertainty: Applications to Publicly Financed Project. *European Journal of Economics, Finance and Administrative Sciences,* 27, 94 - 110

Kokshagina O., Le Masson P., Weil B., Cogez P. (2012). Risk management strategies in a highly uncertain environment: Understanding the role of common unknown. *The 19th International Product Development Management conference, 2012*.

Le Masson P., Hatchuel A., Weil, B. (2010). Modeling novelty-driven industrial dynamics with design functions: understanding the role of learning from the unknown. *International Schumpter Societe Conference 2010 on Innovation, Organization, Sustainability and Crises. Aalborg, June 21-24, 2010.*

Le Masson, P., Cogez, P., Felk, Y., Weil, B. (2012). Revisiting Absorptive Capacity with a Design Perspective. *International Journal of Knowledge Management Studies, Accepted, to be published.*

Legauy, A., Rousseleau, Da. (2011). Conception des couples {Technologie, Marché} dans les cas de la recherche avancée chez STMicroelectronics. *Rapport de stage d'option*.

Lenfle, S. (2011) The strategy of parellel approaches in projects with unforseeable uncertainty: The Manhattan case in retrospect. *International Journal of Project Management* 29, 359 – 373.
Loch, C. L., De Meyer, A. and Pitch, M. T. (2006) *Managing the unknown: A new approach to managing high uncertainty in projects.* New York: Wiley.

Loch, C. L. and Kavadias, S., (2008). *Handbook of New Product Development Management.* Elseveir Ltd.

Loch, C. L., Solt, M. E., Bailey E. M. (2008). Diagnosing unforseeable uncertainty in a new venture. *The Journal of Product Innovation Management*, 25, 28 – 46

Lynn, S. G., Akgun, E. A. (1998). Innovation strategies under uncertainty: a contingency approach for new productndevelopment. *Engineering Management Journal* 10(3), 11–17

McGrath, G. R. (2001). Explorative learning, Innovating Cpacity and Managemet oversight. *Academy of maangement journal* 44(1), 118 – 131

Maidique, M., and Zirger, B. (1984) A study of success and failure in product innovation: The case of the U.S. electronics industry. *IEEE Transactions on Engineerin Management*, 31, 192-203.

Meyer, M. H. and Utterback J. M. (1993). "The product family and the dynamics of core capability." *Sloan Management Review* Vol. 40((2)): pp. 29-47.

Moore, G. E. (1965). Cramming more components onto integrated circuits. *Electronics Magazine*. p. 4.

O'Connor, C. G., Ravichandran T., Robenson, D. (2008) Risk management through learning: Management practices for radical innovation success. *Journal of High Technology Management Research*, 19, 70-82

O'Connor, C. G. (2006). Organizing for Radical Innovation: An Exploratory Study of the Structural Aspect of RI Management Systems in Large Established Firms. *Journal of product innovation management*, 23, 475-497.

Oriani R., Sobrero M. (2001) Market Valuation of Firms' Technological knowledge: A Rela Options perspective. Extended Abstract. *21st Annual International Conference of Strategic Management Society* 2001

Petit, Y. and Hobbs, B. (2011). Project portfolio in dynamic environments: Sources of uncertainty and sensing mechanisms. *Project Management Journal* 41(4), 46 – 58.

Pich, T. M., Loch, H. C. and DeMeyer, A. (2002). On uncertainty, Ambiquity and Complexity in Project Management. *Management Science* 48(8), 1008 – 1023.

Sanchez, R. (1996) Strategic product creation: Managing new interactions of technology, markets, and organizations. *European Management Journal*, 14(2), 121 – 138.

Sanchez, R., B., Bourgault, M., Pellerin, R. (2009). Risk management applied to projects, programs, and portfolios. *International journal of managing projects in Business*, 2(1), 14 – 35.

Sanderson, S. and Uzumeri, M. (1995) Managing product families: The case of Sony Walkman. *Research policy*, 24.

Sommer, C. S., Loch, H. C., Dong J. (2009). Managing complexity and unforeseeable uncertainty in startup companies: an empirical study. *Organization Science*, 20, 118 – 133

Sommer, C. S., Loch, H. C. (2010). Selectionism and learning in Projects with Complexity and Unforeseeable uncertanty. *Management science*, 50(10), 1334 – 1347.

Van de Ven, H. A. Polley, D.. (1992). Learning while innovationg. *Organization Science*, 3(1)

Yin, K. R. (2003). "*Case study Research: Design and methods*". Sage publicatons. 181